

the auroral display, the extremely singular phenomenon which has been described by several of your correspondents. It looked exactly like a white cloud, about 20° long and 2° wide, tapered somewhat from the middle to each end; but it was more luminous than a cloud could well have been at that time. When first seen, its nearest end may have been 30° east of the moon. Its length was nearly parallel to the horizon, and continued so till lost sight of about as much to the west of the moon; and its passage over an area of some 80° occupied probably less than a minute. It passed very near to the moon, but I cannot say whether over it or not.

CHARLES J. TAYLOR

Toppesfield Rectory, Halstead, Essex, Nov. 25

FOLLOWING up my last week's letter concerning the electric meteoroid, if one may so term it, of the 17th inst., I have sifted all the testimony within my knowledge, assigning a numerical weight to each report from internal evidence of its probable value, and correcting for latitude where the altitude of the moon was made the standard of comparison. With data so precarious, and triangles so ill-conditioned, the results can of course only be regarded as a very rough approximation to the truth; for what they are worth, however, they are as follows:—1. That the course of the meteoroid was about S. 70° W. Probably it was 71° 45', the complement of the magnetic declination. 2. That there was a proper motion of a little more than a mile a minute. 3. That the path was vertically over a line upon the earth's surface, whose least distance from Greenwich was 72 miles. 4. That the actual elevation was 44 miles. On this reckoning the body would seem to have crossed in the zenith in North Belgium, the Boulogne district, Cherbourg, and the north coasts of Brittany.

STEPHEN H. SAXBY

East Clevedon Vicarage, Somerset, November 28

My observation at Ramsbury, near Hungerford, was to the effect that while watching the northern aurora, my attention was called, at ten minutes past six, to this monster meteor, then slowly approaching in a direct line to the moon, which was shining most brilliantly. It seemed to pass exactly over the disc, and reappeared on the side, much reduced in size, as if going away from us; and at a distance of about 6° from the moon, scarcely seemed to measure more than 5° in length, it being then about 6h. 8m., which corresponds with the position over Sidmouth at that time. It was very definite in form, like a torpedo. I estimated its length at 15°, and 3° in breadth. I hope to have a hand-made photograph of its appearance ready for publication, by the Autotype Company, in a few days, and on the same sheet is a hand-delineation of the great comet to the same scale.

ALFRED BATSON

The Rookery, Ramsbury

Lavoisier, Priestley, and the Discovery of Oxygen

In the last number of this journal my friend Mr. Tomlinson has criticised my observations on the respective claims of Lavoisier and Priestley to the discovery of oxygen. Without examining, or attempting to refute one of my arguments, and without the citation of any warrant, or authority, he has stated his opinions with an asseveration worthy of a 15th century Professor of Dogmatic Theology. His letter consists of five general statements, and nine dogmatic assertions. I have endeavoured to show that of the former, two are self-evident truths, or at least universally-admitted conclusions, while the remaining three are misstatements; and that of the latter five are completely erroneous, while three are open to question, and one is correct.

1. The universally admitted conclusions are:—(a) that "chemistry has no nationality," and that "discoverers are mutually dependent." Nothing that I have said can possibly be construed into the expression of a shadow of doubt concerning the truth of either of these statements.

2. The three misstatements are that (a) I have "thought it necessary to revive the old oxygen quarrel," (b) that I have "taken an unpatriotic part against Priestley," and (c) "endorsed the complacent statement of Wurtz, that chemistry is a French science founded by Lavoisier." If it be reviving a quarrel and acting an unpatriotic part against a man, to show that by the light of evidence hitherto overlooked one of the greatest scientific men of the last century has been unfairly accused of dishonesty, I am quite willing to be considered unpatriotic and a

quarrel-monger. As to endorsing the statement of M. Wurtz, all I say is that he did not say it "without reason." Many people regard the assertion as quite unreasonable. I confess I do not, but at the same time I do not mean to say that I fully accept it.

[As to my "forgetting, perhaps, that the title 'La Chimie Française' was invented by Fourcroy, and objected to by Lavoisier," I may say that I do not see that this bears the least upon the question. Lavoisier's own words are "Cette théorie n'est donc pas, comme je l'entends dire la théorie des chimistes français, elle est la mienne, et c'est une propriété que je réclame auprès de mes contemporains et de la postérité." (*Œuvres de Lavoisier*, tome 2. 1862, p. 104.) Dr. Thomas Thomson (*Hist. of Chem.* p. 101, vol. ii.) says, "Lavoisier's objection, then, to the phrase *La Chimie Française*, is not without reason, the term *Lavoisierian Chemistry* should undoubtedly be substituted for it." But this does not affect the question whether or no chemistry is a French science as M. Wurtz puts it, for surely Lavoisier was a Frenchman of the French. I say nothing, however, as to the justification of the remark that chemistry is a French science.]

3. "That the compound is always equal to the sum of its elements was known long before Lavoisier" remarks Mr. Tomlinson. I have nowhere asserted that it was not, but the statement is new to me, and I should like to have references.

4. . . . "So early as 1630 Rey gave the true explanation of the increase of the weight of metals by calcination." Any one who will take the trouble to read through Rey's essay "*sur la recherche de la cause pour laquelle l'estain et le plomb augmentent de poids quand on les calcine*," cannot fail to observe how very vague his ideas on the subject were. He indeed attributed the increase of weight to thickened air (*l'air essé*), but the following, as I have elsewhere stated, seems to have been his mode of reasoning:—Air possesses weight; it may be produced by heating water, which during distillation separates into a heavier and a lighter part; hence as air approximates to a liquid nature, it may be supposed to be separated into a heavier and a lighter part by the action of heat; now the heavier part (the "dregs") of air is more nearly allied to a liquid than air, for it has assumed a "viscid grossness," and this part attaches itself to calces during the process of calcination, and causes such of them as possess much ash to be heavier than before calcination. If we calcine a vegetable or animal substance there is no gain of weight, because the assimilated thickened air weighs less than the volatile matter expelled by heat; but in the case of a metal the assimilated air weighs more than the volatile matter expelled, hence there is a gain of weight. Thus he imagined that all calces, from a vegetable ash to a metallic calx, attract this thickened air. It can scarcely be said that a man with these extremely crude notions "gave the true explanation of the increase of weight of metals by calcination."

5 and 6. "Lavoisier's note of 1772 was, as he admitted, based upon Priestley's earlier experiments, begun in 1744." I can nowhere find in Lavoisier's writings any admission of the kind alluded to. (Will Mr. Tomlinson give references?). On the other hand, I do find a note by Lavoisier at the end of Chap. VI. *De la calcination des métaux*, published in the *Opuscules Physiques et Chimiques* (1774), (*Œuvres*, Vol. I., p. 621), in which he says, "Je n'avais point connaissance des expériences de M. Priestley, lorsque je me suis occupé de celles rapportées dans ce chapitre. Il a observé, comme moi et avant moi, . . . &c., &c." This would seem to sufficiently disprove the former statement.

Mr. Tomlinson speaks of Priestley's "earlier experiments begun in 1744." Now Priestley was born in 1733, and although no doubt a clever fellow he certainly did not begin to experiment at eleven years of age! His first paper on gases was published thirty-nine years later, viz. in 1772.

7. That "the acceptance of Lavoisier's doctrine was mainly due to the capital discovery of the composition of water by Cavendish in 1784," I utterly deny; and if desirable will show cause why. Nevertheless, as it has been so asserted, we may, for the present at least, regard it as an open question.

8. Mr. Tomlinson calls Black, Priestley, and Cavendish, "the founders of pneumatic chemistry." Surely John Mayor and Stephen Hales have a better right to the title.

9. "Priestley discovered oxygen in 1774." This, no doubt, is true in a sense because everybody says so. If it means that he got a gas from red oxide of mercury it is true. But let us not forget:—(a) that he discovered it by a random experiment, "by

accident" as he confesses; (*b*) that he regarded it as air containing nitrous particles; (*c*) that he remained in complete ignorance of its nature till March, 1775, before which time Lavoisier was well acquainted with its principal properties, and had recognised many of its functions.

10. "Cavendish discovered hydrogen in 1784." On the contrary, he described it in his "Experiments on Fictitious Air," published in 1766.

11. "Davy abjured Lavoisier's *principe oxygène*, and by his numerous discoveries gave the chemical edifice so rude a shake that it had to be taken down and rebuilt." From our point of view, *in spite of* the numerous discoveries of Davy, the edifice erected by Lavoisier, and which is still standing, had not to be taken down and rebuilt, except in one small part. The theory of acidification was a small part of Lavoisier's labours, and it was Berthollet who called chlorine *oxy muriatic acid*, and who thought that he had proved it to be a compound of muriatic acid and oxygen.

12. Mr. Tomlinson after asserting that "chemistry has no nationality," and "that discoverers are mutually dependent," goes on to say with strange inconsistency that chemistry "had no proper existence for us until Dalton discovered its laws." Surely this is almost as if he slightly altered the "complacent statement of Wurz," and said, "Chemistry is an English science; it was founded by Dalton of immortal memory." We do not think that many will differ from us when we say that chemistry was a science long before the time of Dalton.

Thus we have endeavoured to show that of the nine dogmatic assertions given above (numbered 4-12):—*one*, viz. 9, is correct; *three*, viz. 7, 8, and 11, are open to grave question; while *five*, viz. 4, 5, 6, 10, 12, are altogether erroneous.

There is no possible excuse for us to remain any longer in ignorance of the mighty works done by Lavoisier. The fine quarto volumes, 1862-1868, published by the French government, are a fitting monument to the genius of the man. The petty jealousies which disfigure the history of science during the end of the last, and commencement of the present century, ought to find no place in our minds. The Republic of Science is large enough for every man to receive his due.

G. F. RODWELL

The Comet

IT would scarcely perhaps be civil to take no notice of Mr. Backhouse's letter in NATURE, vol. xxvii, p. 52, the object of which seems to be principally to discredit my account of the disappearance of the comet in a moonlit sky. Still less, however, would it be reasonable to take offence at it—albeit, Mr. Backhouse is wrong. Indeed, a little more reflection might have shown him that ample time having elapsed without any correction from me appearing in your columns, the presumption must have been strong that I had nothing to correct. I have in fact seen the comet frequently since—as well as many times before—and am moreover really experienced enough not to have made quite so gross a blunder; or at least to have found it out, if I did make it, when so many subsequent opportunities permitted. Besides that, I have fortunately the following testimony in corroboration. One of my sisters wrote, "What you did not see of the comet agrees exactly with F.'s experience. She looked out at Court-Lodge: splendid night; many, even small, stars, though moon shining bright; but the comet *wasn't to be seen*, though she and Miss B. scanned the whole fine expanse of east and southeast sky." Another wrote about the same time that though visible two days later, it was so pale that she did not wake a nephew who wished to see it. My drawing of the 23rd October has two stars above the nucleus, with one of which it made the base of an isosceles triangle, the other being at the vertex. These two stars were plainly visible all the morning of the 30th, but not so high above the roof across the way, but what the motion of the comet since I last saw it (23rd) *may* have lowered it enough to conceal the nucleus. In fact, either I am wholly right as to the disappearance, nucleus and all, under moonlight, or at least the nucleus must have been concealed. There is no other alternative. As to the great sweep of tail—let us be reasonable in our guesses as to the fallibility of others however improbable their evidence. May not something for instance be ascribed to the London atmosphere as likely to increase the amount of moonlight reflected? It was for this that I wished the observation made public, viz. as a real phenomenon having a real cause; all the more interesting that it was so surprising—nay, as it seems, so incredible. My only regret is that I have been now tempted into so long a reply.

Before I leave the comet, may I presume to express my surprise that the question as to this comet's return is still *sub judice*. It is said that three well observed places are enough to determine the elements of a comet's orbit. But this one has surely furnished more nearly a score since its perihelion, to say nothing of those before—which no doubt belong to a previous orbit. It is not without fear that I may be misunderstood, that I ask of those who are skilled in such things for an explanation, knowing that of all men they are most deeply interested in the early solution of such a question. It may be said that the observations at and about the time of perihelion have scarcely yet reached this country; but is not the fact that the comet was at one time, which I imagine is known with some certainty, *behind the sun's disc*, equivalent to an observation of its place sufficiently exact to rank with others in calculating the orbit? I do not presume to say that it is so. I merely formulate a question which, in its general bearing, must surely be agitating the minds of many besides myself, after all we have read about the possible past history and future fate of this remarkable comet. It has now been under observation during two months, in which time it must have traversed nearly one quarter of its entire orbit, if an elliptical one of moderate extension. Its present path in space must be so nearly straight that continued observation can hardly be expected to furnish improved data until, if ever, departure from that shall settle the question decisively in favour of an elliptical path. But is it for this that we must wait? I can hardly think so, for surely no comet has ever yet been *seen* in the neighbourhood of aphelion.

J. HERSCHEL

30, Sackville Street, November 18

An Urgent Need in Anthropology

BOTH zoology and geology possess a yearly "record" of the work achieved in their respective domains, but anthropology still remains without that aid to its proper advancement. All workers are of course cognisant of the current bibliography given in the German anthropological publications, and the supplemental information on the same subject contributed by Dr. O. Mason in the *American Naturalist*, and are not unappreciative of the same; but these lists are but partial, and necessarily incomplete, as must be evident when the peculiar nature and wide scope of the study of man is taken into consideration.

Compared with anthropology, the record of zoological work is simple in the extreme. Zoology possesses its accredited organs and regular channels of publication, and with trifling exceptions, its yearly work can be gleaned from these sources. But what is anthropology? It may be described as the very Talmud of humanity with its "Mishnah" of ethnological facts, and its "Gemara" of anthropological conclusions. Scattered up and down the bye-ways of literature, here and there recorded by the traveller, illustrated by the historian or accentuated by the essayist, hidden in blue-books, and awaiting extraction from medical reports, existing in the papers of the missionary and the publications of the statisticians are the unaccumulated and unrecorded facts and observations which form the foundation on which to rear a complete science of man. Our own savages afford as excellent illustrations of the comparative in civilisation as do the primitive peoples of the jungle or the swamp, and hence a large fund of information is still to be supplied and tabulated from our city alleys, prisons, and lunatic asylums. To the question, Is such a record needed? must be added, How is such a record possible?

It seems at once clearly impossible that such a work could be either intrusted to the care of one man, or to the men of one nationality. No individual can be expected to have perused the whole current literature of his country, and could such a phenomenon be discovered, it is still more unlikely that he would combine in himself those qualities which are necessary to detect the varied data that make for anthropology. An alternative course, however, is present, which is possible, and not too exhaustive as regards time and labour. In each country where anthropology is cultivated as a science, a few of its votaries could form an association for the purpose of abstracting from its literature such facts, arguments, and observations as appertain to the study of man, and these might, in a condensed and tabulated form, appear as a regular yearly contribution in the pages of the different publications of the varied ethnological and anthropological societies which now embrace so many nationalities. It is perhaps not presumptuous to say that these papers would not be the least valuable in the volumes in which they appeared. It seems work that anthropological societies might